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REFLECTIONS

ON

Professor Rutherford's Experiments

ON THE

BILIARY SECRETION OF THE DOG.

BY

A. DE NOË¹ WALKER, M.D.



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ADVERTISEMENT.

I entertain the hope that this, the first of a series of papers on Vivisection and other experiments on living beings, will, notwithstanding many imperfections, tend to dispel the falsehoods and delusions guilelessly believed in by many about this dreadful practice. There are however, certain classes, or perhaps I should say, types of persons, not excluding women and "divines" of high order, who appear to be incapable of real pity. They may indeed, when they hear or read about the cries, screams, and struggles of the vivisected, observe, "how dreadful it is," but they experience no unhappiness or even uneasiness, and forthwith, so completely dismiss the subject from their mind, that they would hardly deem it worth their while to walk across the street even to mitigate the evil. To these and to others noticed below, these papers cannot be of any use. Repeated observation has proved to me, that certain characteristics invariably distinguish these people. A certain vulgarity, for example is never absent. What confirms this is that pity for the dreadful sufferings of the animal creation, leading as it ever does to an habitually chastened state of soul, even in the lowest and least cultivated classes, renders them totally devoid of vulgarity, and even refined. Another characteristic of those who can occasionally talk about pity, but have none, is a remarkably restricted benevolence and sympathy that never goes beyond certain determined limits unremittingly under the control of their selfishness. There is nothing spontaneous about them, and while they affect to believe, or really believe they love somebody or something for its own sake, they soon show you they are deceived. If asked if they can believe it righteous that a score or two of living beings should be vivisected in the hope of finding something good or useful for themselves; or whether vivisection has ever revealed a single remedial agent for any one disease, they at once recur to the vocabulary of slaves, and give you to understand that they place themselves under the easy yoke of "those who ought to know best." Vivisection does not interfere with their comforts and peace of mind, and hence they prefer to think and to judge by proxy. My advice to the reader is—from such turn away.

Another class to whom these Papers can be of no use, is the greater number of qualified medical men throughout the country. They read or know little or nothing about Vivisection, but the dreadful *Camorra*

that enslaves their judgment, feelings, and practice, either keeps them silent, or induces them to support Vivisection. "A timorous man you may be almost sure will be on the safer side; a covetous man will bend to that party where gain is to be had; an ambitious man will close with the opinion passing in court; a careless man will comply with fashion; affectation arising from education or prejudice will hold others stiff. Few do follow the results of impartial contemplation." *

Cannot half-a dozen physicians be found that will courageously stand up and vilify the direful *Camorra* that degrades the medical profession?

To a third class of individuals, nothing said or published on the subject can possibly prevail, or in any wise avail. This class or type has no mercy. Their ferocity knows no bounds. They do not know enough about Vivisection to judge for themselves, but they listen with eagerness to all they hear in support of the practice. Their dominant idea is, that they wish to live, and to live as long as possible, free of bodily discomfort no matter how many living beings are vivisected or otherwise tortured. They preach that all available means must be sought for to obtain that end, and that Vivisection should be practiced even for the attainment of manual skill. Two sparrows were caught and sold for one farthing, The Apostles to whom this was said, were worth more than many sparrows, hence this merciless type of human beings preach that they have a natural right to *vivisect* as many sparrows as they like. Miserable sinners indeed; but if blessed are the merciful for they shall obtain mercy, it will be equally true, that "he shall have judgement without mercy that hath shown no mercy."

Another hope I venture to entertain is, that this and subsequent publications will encourage and strengthen those who stand up as witnesses against the worst phase that human selfishness has ever assumed.

I trust also that the next and following Papers will be less wearisome than this one is likely to be. It might have been made less so, had time and tranquility permitted. Indeed, had it not been for the aid afforded by an American Type-Writer, the attempt even, could not have been made.

It only remains for me to add that I believe, with one exception perhaps, that all the italics are mine, and that I trust all the quotations have been accurately transcribed. If however, in any case, I have been inaccurate or unfair, all will be acknowledged and rectified in the next Paper.

ARTHUR DE NOÉ WALKER.

* Isaac Barrow, 1683.

THE many and cruel experiments performed at Edinburgh by Dr. Rutherford, and by his two assistants Messrs. V. & W. Dodds, on what they believe determines the biliary secretion of the dog under the influence of certain agents, are not only utterly un-scientific, but so obviously senseless, that all who have been providentially endowed with the ordinary measure of sense, common to mankind, will turn away from them with feelings of righteous indignation, and of unalloyed contempt. Only the idolators who have been carefully trained to believe and to develop the traditional errors, venerated in their laboratories and lecture halls, would ever assume, that to force the functions of a dog's liver, by the unnatural method and means employed by Dr. Rutherford, can represent that which can ever occur, when the corresponding organ in man is stimulated by an appropriate cholagogue. But Dr. Rutherford and his two assistants are so far from apprehending this, that in defiance of all that nature sets before us, as her own proper way of proceeding, they force an agent to act on the wretched animals, in ways and in conditions, so violent and un-physiological, that the "Experiment" not only cannot represent that which can occur *in the human being*, but does not even prove how the agent really might act, physiologically, *on the very animal experimented on*.

No science, no learning, only common sense, is all that is necessary to enable the public to perceive the folly of calling these cruelties, "experiments." All sorts of material effects, accompanied by all sorts of phenomena, may, of course, be provoked by all sorts of means and methods, but unless the results are physiological, or in other words, unless the results are elicited by such means as shall, as it were, invite and permit nature to act in her own way, they cannot even lead

us from the unknown to the unknown. They can only delude us, or rather we shall only delude *ourselves*, into solemnly grafting a new error on an old one, and thus, perhaps become the famed, but really contemptible means of laying the foundation of a new *system* of delusions.

A physiological experiment is an experiment that consists in eliciting phenomena and effects that result from the normal order of nature. Both to illustrate and to prove this, it is only necessary to take but a superficial view of the method followed by Dr. Rutherford in performing on his dogs. I call them performances, or place the word "experiment" between inverted commas, because they are not physiological experiments at all. They are rude attempts to induce nature to suspend her ways and immutable laws, to suit a vivisector's aberration from common sense.

The first part of the performance consists in making the animal fast from seventeen to nineteen hours. At about 9 a.m., it is brought into the Laboratory and weighed, and the number of kilogrammes indicated by the machine is duly recorded, that the dose the animal is to get may be proportioned to its weight. This sounds specious, and even important, but when a dog or human being is unwell, the dose is not prescribed according to their respective weight; it would be very inconvenient if it were so. A dose, and the frequency of repeating it, depends on the nature of the medicine prescribed,—on the violence and nature of the morbid state,—individual habits, diatheses, and age, etc.; conditions which greatly vary, in every possible way, in every country and season, developing and involving organic changes and states in human beings, on which none of the ill-treated and innocent brute creation, whether in health or disease, dead or alive, have ever thrown, or can ever throw any therapeutic light whatever.

After the animal has been weighed, Dr. Rutherford and his assistants lay hold of it, or tie it down at once, in order to open a vein, into which curare is injected, to prevent struggles and cries. And because in some of the "experiments" the dog, the poison, or both, get exhausted, the dose has to be repeated more than once.

Another operation is now necessary to keep up respiration. This is done with a pair of bellows through an aperture made in the wind-pipe. This operation may be done before the animal is paralysed. An incision is next made in the middle line, and a tube is inserted into the bile-duct, into which an opening has been made. The cystic duct is now occluded with a clamp, an opening is now made in that part of the intestinal canal called the duodenum, and a cholagogue, or some other substance to be tested is injected into it. The "experiment" is now said to begin, and to those whose feelings and conscience have not been seared with a hot iron, the sight of the miserable and helpless victim would be intolerable.* Now, bearing in mind again, that no one, however sincere and earnest he may be, can, while in the pursuit of any science or occupation, dispense with common sense, I would ask, if any unsectarian human being, endowed with an ordinary measure of it, can believe that this ought to be accepted as a "physiological experiment"? Is it not thoroughly un-physiological? Can the results put before us, I do not say, the natural order of things in *man*, but in the very dog experimented on? Starved,—Thirsty,—Curarised,—Vivisected,—the cholagogue not sent into the stomach, which is the place nature made for it, but injected into the duodenum through an opening made into it—Not allowed to move about in his natural horizontal position, but kept motionless for many hours, until the animal is so exhausted by pain, thirst, and want of food, that nothing more can be got out of him—None I say, save those who have parted with their common sense, to make room for some fantastic theory emanating from some Sect or Association could believe that either the method or the process described can represent anything in the order of Nature, and that can, therefore be put into practice? The vivisectors, however call it experimental physiology! But if this violent mode of proceeding, does not, and cannot, show

* Professor Sir R. Christison thus estimates the relative amount of sufferings endured by Dr. Rutherford and his wretched dogs. "I do not say that these experiments were not cruel; but I will put against the cruelty the nauseousness of them *to the investigator*." I leave it to the Public to decide if it is decent, especially for an old man, to talk in this way of the dreadful sufferings of 70 or 80 dogs. Tho "nauseousness" did not prevent the "experimenter" enjoying his meals, while the poor innocent animals were kept motionless,—Thirsty,—Starved,—and in dreadful pain, till "amiable death" came to their relief.

us how a dog's liver is physiologically that is *naturally*, acted on by any agent whatever, how can it represent anything that is likely to take place in a human liver? Even the Homœopathists who base their theory and practice on the action of remedial agents on the healthy organism, would scout the idea of Dr. Rutherford's "provings" being otherwise than useless and deceptive. But so utterly violent and unnatural are Dr. Rutherford's attempts to search for knowledge, that even if his experiments had been made on the *human subject*, the phenomena would set before us the effects of a process that never occurs in man, no more than it occurs in dogs, except in his "physiological" Laboratory. When a cholagogue is prescribed to a patient, he is not curarised, and his respiration is not kept up with a pair of bellows. An incision is not made in his hypogastric region, to expose the duodenum in order to make a hole into it, through which the medicine is to be injected. When a patient has ingested a cholagogue, in Nature's own way, it goes into the stomach, where it undergoes who knows what "chimico-organic" changes, when it mixes with the food, and is otherwise acted on by the secretions there, acid and alkaline. Moreover, the whole human organism, and the human liver in particular, is acted on throughout life, by an immense variety of edible and potable materials, varying from ten to twelve, to as many as twenty or thirty in the twenty-four hours; while a dog's daily food may consist only of flour and water, or at most, of meat and bones, to the almost total exclusion even of common salt.

The "reasons" adduced by Dr. Rutherford, for injecting the drug, not in the stomach, but in the bowel, through an opening made into it with an instrument, are, of course, in perfect harmony with the whole of his unnatural mode of proceeding. If the first step that initiates a process is false, and the mistake is either ignored by the experimenter, or not perceived, it must follow as an inevitable consequence that all the subsequent stages of the experiment are forced to harmonize with the initial error. Thus, he in the first place informs us, that because the empty stomach of a dog contains a considerable quantity of viscid saliva, that would envelop and entangle the drugs and retard absorption, he injects them into the bowel. Then why make the animal fast? Because, says an experimenter, the liver secretes more bile when

food is not being absorbed. But if, when either a human being or a dog is dosed for some ailment, neither of them are made to fast, why experimentalise on an animal in a horribly complicated and abnormal state, that does not occur in Nature, and is never recurred to in practice?

Another "reason" offered for our acceptance by Dr. Rutherford for injecting the drugs into the intestine, is that it allowed them "a fair opportunity of exciting its mucous membrane and thus reflexly stimulating the liver, did they possess any power of doing so." But when a medicine is administered to any living creature, to say nothing about the starving—curare and vivisection,—their liver is never acted on or "excited" in such an unnaturally *direct* way. It is swallowed and deposited into the stomach where it acts,—and is there, itself, directly, and indirectly, acted on, by the secretions and nervous centres, all which influences may permit, modify, or altogether prevent the action of a drug on the liver, or on any other viscus whatever. I beg the reader not to assume, that because I thus object to these experiments, I would recommend pharmacological experiments on animals to be conducted in some other way, or in any way whatever. All such experiments, are not only cruel and misleading but *greatly* retard therapeutic science. "Those who look for the results of experiments on mutilated animals, in the following pages, will not find them, for the Editor is satisfied that this is not legitimate therapeutic inquiry, and that nothing short of a patient survey of the operation of a drug in the *entire* body in health, and under the variable influences of disease, can furnish the data upon which we may build a proper *theory of its action*."*

Before some of the "experiments" are examined in detail it may be well in a few words to recapitulate what I have endeavoured to make clear to the general reader.

Dr. Rutherford tests the action of drugs in a dog's body in a thoroughly un-physiological, that is unnatural way; in a way that is never recurred to, when any therapeutic agent is administered to any living creature. How can it there-

* Preface to the sixth edition of Royle's *Materia Medica and Therapeutics*, By John Harley, M.D. The italics are mine.

fore set before us the natural action of the remedial agent in the animal at all? It only sets before us, how the remedial agent acts in the poor animal, when paralysed, vivisected, starved, and the agent has been injected, not in the stomach, but in the intestine, or in a vein. It does not show us how it would act in Nature's own way on the dog at all.

Secondly. If the violent and unnatural way in which the experimenter tests a cholagogue on a dog, could, and really *did* show us its real action on the healthy *dog*, it would teach us nothing about its physiological action in *man*, because if the same experiment were performed on a healthy human being, he would not be made to fast and thirst for twenty-four hours—he would not be paralysed with curare, and his respiration would not be kept up with a pair of bellows—he would not be vivisected—and the medicine would not be injected into the intestine, but would be naturally deposited in the particular organ where assimilation commences, physiologically. Analogy between the two experiments can only be subjectively conceived by those who “seeing they see, but do not perceive.”

Thirdly. If the experiments were even in some measure physiological, the cholagogue, in *any* case, could only have acted on the *healthy* liver of the animal, and the factitious results, even if they were worth anything, would not afford an indication as to how the cholagogue would effect the *diseased* liver of a dog. And if the “experiments” do not show us how “cholagogues” act in a natural way, either on the healthy or diseased liver of a dog, how can they at all indicate the therapeutic value of those agents in treating diseased states that do not even occur in dogs at all?

In the hope of helping the public to mature their judgment on the dreadful way in which our animals are treated, a few of Dr. Rutherford's performances will now be analysed. I put it thus, because, in the first place, the time has come that the public should bear in mind, that every living creature, of the brute creation in these Islands, has a right to be fairly treated and amply protected. And secondly, because the sufferings the poor creatures are made to endure by the vivisectors, has become a public question, involving humane and moral considerations which ought to concern the public conscience.

I will begin with the experiment on hydrastin, because it bears out that which I hope I have made clear, viz.,—That if the experimenter had succeeded even in some degree, in proving the action of a cholagogue in a really physiological way on a healthy liver, it would not prove how that agent would act on that same organ, *not* in a healthy state.

ACTION OF HYDRASTIN.

“Dog had fasted 17 hours. Weight, 13lbs. 6oz. Two centigrammes of hydrastin, triturated with two centigrammes of bile, one centigramme of rectified spirit, and six centigrammes of water, were injected at two different periods, in two portions of the duodenum.”

The experimenter does not deem it necessary to test the hydrastin by placing it in the stomach of the animal. He prefers putting the dog in conditions thoroughly un-physiological, and then inject it into the intestine, as already and so often described. It is here again adverted to, simply to bring out in relief the fact, that one un-physiological proceeding necessitates another and consecutive one, equally un-physiological thus, *e.g.*, when hydrastin is prescribed to a human being, it is not mixed with bile, spirit and water, and no one save a worshipper of erroneous academical traditions would assume that hydrastin not mixed with bile, spirit and water, and deposited into the stomach of a human being, there to undergo who knows what series of changes, would produce the same results, as when the same hydrastin is mixed with bile, spirit and water, and injected into the intestine of a dog. To an idealist carrying out certain notions of his own, for some Association or party, this lack of analogy is of little, or perhaps, even of no consequence.

In noting down a short summary of this “experiment” for his employers, the experimenter says, “Before the experiment was begun, it was observed that the animal was somewhat *unhealthy*, which accounts for the result being less definite in this than in the former case. The fact shown in Table IV that in Experiment XIII a dose relatively *larger* in proportion to the size of the animal than in Experiment XII produced a *smaller* effect on the liver, seems only explicable by the fact that the subject of the former experiment was, as already stated, in an *abnormal condition*.”—*British Medical Journal*.—January 11th, 1879.

We do not of course know what the experimenter might have inferred or did infer, about it all, but as far as we can judge, I believe we are justified in saying, that the way he contents himself with noticing the bare facts, constituting so essential and important a difference between the two "experiments," is quite consistent with the mental perceptions of one accustomed to question Nature in such rude, violent, and unphysiological ways. A certain substance is given to a dog *in health*, and a certain result ensues. The same substance is afterwards given to another dog, somewhat *out of health*, followed, of course, by a modified result, but which is declared to be "less definite." I share the convictions of those who believe that not only the habits of thought, single-eye, and common-sense of the professional vivisector are so perverted and obscured by the violent habit of attempting, for the greater part of his life, to seek for therapeutic knowledge where it does not exist, that it would have been indeed surprising, if the simple and obvious truth regarding the two results, had been noted down by Dr. Rutherford in language suggested simply by common sense.* The Public will at once perceive however, that the result of the performance on the sick dog, so far from being "less definite," was, on the contrary, most definite. In one case, the agent found a *healthy animal* to act on, while on the other it was made to act on *unhealthy organs*, with, of course, a modified result.

"Unfortunately," observes Claude Bernard, "I ought to reproach not pure physicians only, with making a false use of physiology and pathology, but I would address myself to pathologists and medical men, exclusively occupied with those sciences. In several recent publications on medicine in which on one hand I praise the physiological tendencies found therein, I have seen, for example, that before purely medical observations are made, a résumé is given of all that experimental physiology has learnt respecting the particular phenomena of the disease in question. Observations are thus

* "Once, in a well-known letter, *Tham*, the French Artist, suggested that the experiments performed on dogs at the College de France, should rather be performed on the distinguished professors who indulged in the pastime. The question, he argued, with some show of reason, would be considerably more conclusive from a scientific point of view, although it may be doubted whether men who can accept the idea of such atrocities have in any degree the same formation as the rest of mankind."—*Truth*, September 18th, 1872.

brought forward regarding disease, void of scientific aim, while at other times attempts are made to show the harmony that exists between physiology and pathology. But beside the fact that this accordance is not easily established, because experimental physiology presents, as yet, many points requiring study, I hold a similar mode of proceeding essentially disastrous to medical science, inasmuch as it subordinates pathology, a more complex science, to physiology, a simpler science."

ACTION OF PHYSOSTIGMA AND ACETATE OF LEAD.

After testing physostigma in a couple of dogs, in the usual unnatural way, that cannot prove either its physiological or therapeutic action on any living creature, and after proving, as the experimenter thinks, the antagonistic action of atropia on the physostigma, still acting on the liver, he informs the Medical Association that "as the action of acetate of lead was to be investigated, eight grains of that substance, dissolved in twenty centigrammes of water were injected into the duodenum, and the secretion of bile soon thereafter, came to a standstill."

But who, save a vivisector, would ever think of "investigating" the action of acetate of lead on the liver of a dog, in which first two grains of the extract of calabar bean had been injected; then four-fifths of a grain of atropia; thirdly, again three-fifths of a grain of the calabar bean; and fourthly, three grains and a half more of the same extract of calabar bean? Anybody else could at once perceive that even if the "experiments" *had* been physiological, no other drug could manifest its own proper action while the whole organism was under the influence of three doses of calabar bean and two doses of atropia. Dr. Rutherford however, tells the Association that "subsequent experiments show that this effect was *unusual*, and attributable" To what? "To the depressent effect of the lead on the liver already well-nigh exhausted." What nonsense!

Finally, we are assured, that "the effect of physostigma on the liver is completely antagonised by atropia-sulphate." The Medical Association of course believe it. Common sense however, not trained to believe in modes of investigation peculiar to "scientific" sectarians, will not believe that the experimenter has proved the action of physostigma, in a

natural way, on *any* liver at all, and in this particular respect therefore, the antagonistic action of atropia remains to be proved.

But in very truth, if Dr. Rutherford's "experiment" really proves anything, it proves the contrary. Thus he tells us, that in his first trial he injected two doses of the calabar bean into the intestine, and the bile flowed freely. In order now to see if atropia is capable of stopping the flow of bile caused by the bean, he injected also into the intestine, four-fifths of a grain of atropia-sulphate and the effect we are informed, "being somewhat doubtful," he then injected six-tenths of a grain not in the intestine, but in the *jugular vein*, and in half-an-hour the bile ceased to flow. This, the British Medical Association accepts as proving the antagonism of atropia on physostigma acting on the liver of a dog; but to all whose common sense has not been obscured by "scientific" trash it proves nothing of the sort. It only proves that as soon as the poison has been injected (not into the intestine as the physostigma had been) but into a vein, and carried thence by the circulating current not to the liver only, but through the whole system, as a natural consequence the whole system was affected by the poison, and the liver, most likely, influenced quite in a secondary way. I have no doubt at all that if any other poison as powerful as atropia, or if both the doses of atropia had been injected into the vein, the result would not have "been somewhat doubtful," but that the flow of bile would have ceased sooner than in half-an-hour.

The Public will now perceive another fallacy in these cruel "experiments." This experimenter injects one thing into the gut, and the other into the vein, and hopes we shall believe that both substances acted in an equally direct way in the animal's body. But common sense will at once perceive that the physostigma, however unnaturally tested, acted directly, far too directly on the liver of the dog, while the other poison influenced the whole organism: heart, blood, brain, spinal marrow, as well as every other part of the system, and then on the liver, in two or three mediate or immediate ways. This fallacy is still more patent in the second "experiment." Two grains of the extract of calabar bean, triturated with one centigramme of bile, and five centigrammes of water, were injected into the gut. "The stimulating effect on the liver

was rapid and powerful." Four-fifths of a grain of atropia with four centigrammes of water but no bile, were now injected into the "jugular vein." "Ere long," says the experimenter, "the atropia asserted its influence and antagonised the physostigma. Atropia amounting to three-fifths of a grain, was again injected into the vein, and it became evident that the physostigma was completely antagonised thereby." Dr. Rutherford has not yet done with his poor suffering dog, and says, "A continuation of the experiment was perhaps scarcely necessary; still a grain and a half of calabar extract triturated with one centigramme of bile and five centigrammes of water was injected in one part of the intestine, and three grains similarly treated, were injected in another part. The exciting effect," we are told, "was not very marked; nor need this to be wondered at" he continues, "considering how powerfully the liver had been previously stimulated, and its partial exhaustion induced not merely to the above cause, but also to the duration of the experiment." No doubt the poor dog, curarised, vivisected, starved, thirsty, and drugged, was not capable of affording us more information about our liver, but the chief reason why the result was "not very marked" was due to the deadly poison having been injected, not in the intestine, but in a vein. Perhaps all this confusion may account for the following not very rational, and not very clear series of assumptions, entitled "*Results of Experiments in atropia.*"

"Atropia-sulphate does not paralyse the hepatic cells, neither does it appear to excite them. Whether or not it has the power of paralysing the hepatic secretory nerves is doubtful; but seeing that it antagonises the effect of physostigma on the liver, and remembering the actions of those substances on the nerves of the heart and salivary glands, the suspicion is entertainable that physostigma stimulates the hepatic cells through a nervous apparatus, that is affected in the opposite sense,—possibly paralysed—by atropia; while the hepatic cells, and possibly some nervous mechanism like the motor ganglia of the heart in close relation to them, and unaffected by atropia." Now I would ask anyone how they would like a doctor who believed in such talk, for it is nothing else, to come to their bedside and prescribe for their liver? But to all who have been taught that words are not things, nor facts but only signs, a feeling akin to despair, will,

after reading the above, inevitably trust itself on the mind. The only effect these experiments can have on the progress of therapeutics is retrogressive. Not I believe that anyone will make use of them and be disappointed, but because, as Bufalini so often remarked, similar delusions take the place of real therapeutic investigation, and thus, as he frequently asserted they do not retard, but arrest therapeutic science. The whole theory on which those performances are based, and carried on, is utterly false, not only because the medical substances have been tried on brutes, and not on man, but because the method of experimentation, whether on dogs or on human beings, is utterly un-physiological; or in other words, thoroughly unnatural. The method, moreover, false as it is, is frequently inconsistent with itself. One substance is injected into the intestine; its antidote or supposed antidote, is sometimes injected also into the intestine, and then in the *same animal*, it is injected into a vein, with different results, while the cause that brought about the two different results is not even noticed. Again: the same antagonistic, or supposed antagonistic substance in *another* experiment, is injected, not in the intestine at all, but in the *jugular vein*; but the same inferences are drawn as when in a previous experiment, the same substances had been injected into the *gut*.

Sometimes two, and even three different substances are ejected into *one animal* in which they of course, meet, change, act, and re-act on each other and on the organism, but the results are all separately noted down, as if each different substance had had a clear field to act on, uninfluenced by other drugs previously forced to act, who knows how, on the whole animal. Nor is that all. Not content with causing a complication of organic changes as just described, the experimenter throws them into the animal through different channels. One or two are injected into the veins; another into the intestine, or *vice versa* but never into the stomach, the organ Nature made for the reception and changes that food, liquids; and remedial agents undergo. Thus, in testing atropia on a dog, in the usual dreadful condition, the experimenter says—"Half a grain of atropia-sulphate, dissolved in 20 minims of water, was injected into the *jugular vein* at a' a", and again at a""; and one grain was injected at a"" figure 3." "Two grains of acetate of lead, dissolved in 20cc., of tepid water, were

injected in the *duodenum* at 1 without producing any notable effect." After this, twenty-five grains of sodium-salicylate dissolved in 25cc. of water were injected into the duodenum, and within half-an-hour a very rapid secretion of bile had begun, and this notwithstanding the previous administration of acetate of lead, and three grains of atropia sulphate." Then follows the confused paragraph above quoted, and who in the world, save a vivisector, would test atropia or acetate of lead on the assumption that they might act as cholagogues? and who, save a vivisector, would assume that the action of sodium salicylate as a cholagogue has been established by the above performance?

The following remarks on Dr. Rutherford's "experiments" on sodium-salicylate will answer the question.

He assures the Medical Association, that his experiments "furnish *abundant evidence* of the remarkable power of sodium-salicylate as a stimulant of the liver." Indeed he continues "it is hepatic stimulant *in the dog* never failing when placed in the duodenum to excite the liver within half-an-hour." "It is a very powerful hepatic stimulant *in the dog*, and will perhaps be found to have a similar effect in the human liver now that attention has been directed to the subject."

One hardly knows how to begin to deal with such stuff. The vivisector says that his experiments furnish abundant proof, of the great power this agent has on the liver of a dog. I cannot myself perceive in what the abundant evidence consists. A veterinary surgoen who wanted to know how sodium-salicylate acted on a dog's liver, would give him a dose three times a day, and would watch the result of both when absorbed in Nature's own way. But Dr. Rutherford starves, curarises, vivisects, and throws the sodium-salicylate into the duodenum, and wishes us to believe that these means furnish abundant evidence that it will perhaps have a similar effect on the human liver!

ACTION OF MORPHIA.

Here is one instance of the utter uselessness of Inspection. Why, if the Inspector knows his duty, and is duly impressed with the sense of his dreadful responsibilities, greater I appre-

hend than those of many other public functionaries, has he not recommended the Home Secretary to withdraw this experimenter's license? he made two dogs undergo dreadful suffering to ascertain whether morphia has "the power of diminishing the secretion of bile." But no student dare go up for his final examination, without knowing right well, that morphia arrests, more or less, all secretions.

ACTION OF CALOMEL.

The experimenter assures the medical profession through the journal of the *British Medical Association* that calomel stimulates the intestinal glands, but not the liver. Here again, if the Inspector knew his business, he would have reported this as well as the foregoing experiment, as useless, cruel, and misleading.

"Rutherford concludes that calomel does not increase the secretion of the bile, nay in purgative doses, it may even decrease it. Yet the *experience of generations* strongly supports the general conviction, that in some diseases, calomel, as well as other preparations of mercury does increase the bile." *Hand-book of therapeutics by Dr. Ringer, 1879.*

"That calomel in purgative doses, increases the flow of bile into the intestines, is a cholagogue, cannot be successfully disputed."—*Practical treatise on materia medica and therapeutics, by Dr. Robert Bartholow. 1877.*

In his concluding observations on what I believe to be his dreadfully painful and useless experiments, Dr. Rutherford observes :—

"We claim that by means of a novel and precise (!) method of investigation, we have been the first to place the whole subject (!!) of the physiological action of drugs on the liver, upon a *sound footing*, and thus to lay a real foundation for the rational, that is, scientific treatment of many diseased conditions of this important organ." I believe the only thing that is true in the above is all that can be connected with the word "novel," and that very likely by this time, most readers whether they believe in vivisection or not, would refuse to have their diseased livers attended to by either Dr. Rutherford, or by anyone who believes in his "precise" method.

"We have, indeed, occasioned by our experiments" he continues "a considerable amount of pain to a number of dogs, but considering that our discoveries (!!) are calculated to relieve much suffering not only in man, but also in dogs, for *all time to come* (!!), we believe that we have spared infinitely more suffering in the future than we have occasioned in the present." What nonsense!

I believe that most readers, and indeed, the public generally, would, in the absence of evidence to the contrary, entertain the hope that while so many poor, helpless and speechless animals, were undergoing so much suffering, the experimenter and his two assistants, and indeed, all in and about the laboratory, would be at least sobered down, into a grave if not into a depressing, sympathy for the poor creatures. The following paragraph, however dispels that idea in an exceedingly disagreeable way.

"In conclusion," says Dr. Rutherford, "I have to tender my warm thanks to my former pupils, M. Vignal, and William Y. Dodds, M.B.D., &c., for their valuable assistance in the performance of my experiments, and for their *agreeable company* during the long and weary hours through which they daily extended."

The following is the last paragraph of Dr. Rutherford's "concluding observations."

"I have cordially to thank the Scientific Grant's committee of the British Medical Association, for having entirely defrayed from the funds of the association, the very heavy expenses incurred for the materials of the research, and for their energetic and powerful support, when the clamour of blind ignorance and silly prejudice seriously menaced and almost arrested the progress of this research."

Having personally devoted not less than *fourteen hundred hours* of severe labour to the accomplishment of this work, and having (as of course, every medical man thinks himself bound to do for the alleviation of suffering) communicated to all, every fact calculated eventually to cure (!) affections so common as those of the liver, it is to say the least, ungrateful, that a certain section of the public, should have

rewarded our unselfish efforts to cure their *biliousness* by a flood of scurrilous abuse, because like most our medical brethren, we refuse to believe, that to be penny-wise and pound foolish as regards pain is not a policy as short-sighted and as narrow-minded and as reprehensible here as elsewhere. Though profuse in their ingratitude I doubt not that one and all of them will be very ready and eager to profit by the results of our labour; for I believe them to be too much recreant and craven-hearted to allow themselves to refuse all medical aid, and thus to push their ill-conditioned logic to its practical issue. Desirous, as I think most of them are to continue in receipt of all the medical assistance they can obtain, they naturally try to overcome their conscientious scruples by vainly attempting to show, *that nothing worth knowing* in medicine, has been learned from experiments on animals."

"It is not difficult, by misrepresentation and by a multiplicity of words to deceive a public ignorant of the machinery of life, and the processes by which its movements are studied, and remedies found for its disorders; but they cannot thus deceive any well-informed and right-minded practitioner. It would be a just return for their opposition to medical experiments on animals, were medical men to take them at their word, and deny them the benefit of advice that is directly or indirectly based on such experiments. Their not having been already placed in this predicament, is entirely due to the long-suffering charity of the medical profession, whose members are slow to refuse to alleviate the sufferings and prolong the lives of those who misrepresent their motives and throw obstacles in their way."

Mr. Cross told the House he did not give the names of the vivisectors he had licensed, because if he did, they would lose their practice. Is that also a misrepresentation?

"But, if they really wish medical men to minister to their wants, the sooner they lapse into becoming silence the better. Meanwhile, heedless of their ignorance so contemptible were it not mischievous their scurrilous abuse—usually anonymous—and their shameful misrepresentations, we steadily have recourse to those methods of research which every medical man worthy of the title, approves, because he knows them to be essential for the advancement of medicine; and the

conviction of earnestly doing our utmost to prosper the cause of rational therapeutics,—that is, of a scientific practice of the healing art,—is to all earnest votaries of medicine a sufficient recompense for labour as honestly unselfish as it is practically un-remunerative. And when the dupes of the anti-physiological,—that is,—of the *anti-medical* apostles, awake to a sense of the great injustice of the present anti-vivisection agitation, I would suggest that they could not do better than liberally contribute to the endowment of the few, too few, physiological laboratories to be found in the country; for it is not reasonable that, while the public receive the benefit, the heavy expenses of a research such as the above, should be defrayed by the practitioners of medicine throughout the country.”

Now what I would particularly wish the public to be aware of is this—Given that all the above, which seems to me at least, to contain more temper than sense, be read or spoken, say, before one hundred medical men, all the professional vivisectors present would applaud it. One section of physicians and surgeons present, without caring to examine the worth or worthlessness of Dr. Rutherford’s performances would think it “their duty” to accept all they heard, under the head of “science,” “rational medicine,” “progress,” etc., and approve of them; just as “the profession” approved of the “scientific way” in which hundreds of thousands of lives were destroyed by mercury, bleeding, blistering and excessive dosing. Another section would not deem the performances worth the trouble of proving them useful or not, and would return to their own practical experience of disease and remedies. And finally, a certain section of thoughtful and studious men, would deem them obstructive and cruel, but would not have the courage to say so. Nevertheless, if the next day, a public meeting were held for the express purpose of declaring Dr. Rutherford’s performances cruel and abominable, the whole hundred could be induced to come forward to declare them “calculated to advance medicine, and prolong human life.” The public can have no idea of how the profession is enthralled by a secret and most intimidating “*camorra*,” whereof the chief supporters are medical journals.

The first person who got up to move “that the best thanks of the Association be given to Professor Rutherford for his able and interesting address on physiology”—was Dr. Burdon-Sanderson, a mighty Christian vivisector before the Lord,

whose estimate of his own heavenly calling, and as a practical witness for his Redeemer, appears to consist in being convinced that—"There is no motive so high as the desire to increase knowledge for the benefit of the human race."

I generally make an effort to avoid bringing religion to bear on the dreadful practice of vivisectioning animals, because, in the first place, irrespective of any creed, to vivisection animals in the hope of finding a natural, that is, a true means and method of curing diseases is thoroughly un-philosophical, and against the order of nature. If it were otherwise, then, if even one remedy for the benefit of the race had been discovered for every ten-thousand animals vivisectioned, the therapeutic results would have been by this time so abundant and comprehensive, that vivisection would have become, long ago, no longer necessary.

In the second place, every one excepting, of course, those whose conscience is only a mere appendage to their profession or trade, whether they believe the N.T. to be a Divine Revelation or not, plainly perceive that vivisection is simply one of the best means possible of practically denying both the very letter and spirit of the New Covenant; while those whose conscience is an appendage to something else that interests them, must think it a pity that the Apostle ever wrote the last 13 verses found in the first chapter of his first epistle to the Corinthians.

"It is," this vivisectioner continues, "by means of such investigations *alone* that they could answer to the question which of all others interested them most in physiology, or in fact, in any of the applications of science to medicine—the question—"How did those agents, of which they had a *vague knowledge* by means of their experience in medicine, affect the great functions of the body"?

I leave it to any impartial reader, gifted with an ordinary measure of common sense, if Dr. Rutherford's performances answer the above question? Twelve human beings, shall each respectively be suffering from:—

Hepatalgia.

Jaundice.

Biliary Calculi.

Suppurative inflammation of the liver.

Gangrenous inflammation of the liver.

Fatty liver.

Cancer of the liver.

Hydatic disease of the liver.

Waxy disease of the liver.

Acute yellow atrophy of the liver.

According to Dr. Burdon-Sanderson, the only way of attaining to a certain, or nearly certain, knowledge of how remedial agents are to act on twelve different morbid states, on twelve different individuals, with different constitutions, different habits, in the various stages of their respective diseases,—is to turn to, and study the effects of those agents on one hundred dogs, starved, curarised, vivisected, and the said remedial agents, not sent even into their stomach, but squirted into the gut or veins, through a hole made with an instrument. The insolence of calling clinical experience of remedial agents, “vague knowledge.” Further on he said :—

“I have myself the most profound confidence in the sagacity of the legislature, not that that body never makes mistakes, but generally when it is well informed, it acts with judgment. All they had to do, was, he believed, to secure that perfect information might be given to Parliament, and then, they might be quite sure that, if there were legislation, the results would be such as would further and protect science, not tend to destruction.” (Applause.)

Many do not estimate from a vivisector’s point of view, the value of how Parliament works for the country. The declining efficacy of Parliament is a subject that has, and is, occupying the thoughts of some able men. As respects strictly legislative measures, its working capacity is exceedingly slow and cumbersome, leaving generally half the work to be done, or not done at all, to another session. The first reform required, and for the present, it might prove sufficient, is that that body should talk much less. That which fills a whole column of a daily paper, might be, and ought to be said in, at most two-hundred words. *

But it would appear rather an easy matter for Dr. Burdon-Sanderson to be profoundly impressed. He assured the Royal Commission that, it was his profound conviction that a future would come, it might be a distant future, in which the treatment of diseases would be really guided by science.†

* My own conviction is that *one* Parliament, as at present constituted, is, as regards the internal welfare of the United Kingdom something approaching to a veritable obstruction.

† This condition is a favourite one with the vivisectors. If vivisection has done nothing yet, the public may thus be encouraged to look forward to some unlimited benefits at *some future time*.

It would be a pity to deprive both the public, and the medical profession of that hope. The means, however whereby Dr. Burdon-Sanderson, has all his life I believe, been endeavouring to develope the science that is to guide therapeutics, in my judgment, do nothing else than both confound the present, and retard that future time in which the treatment of disease is to be really based on scientific knowledge. But has this vivisector *nothing* to give us yet! Cannot he after his innumerable vivisections at least give us one single therapeutic fact, that we may hope will ultimately develop into a guiding truth? I on my part am profoundly convinced that he like M. Claude Bernard will die, and leave not one single remedial agent to cure any one disease; nor indeed any therapeutic item or theory that will change even the *treatment* of any one disease to which mankind is liable. Dr. Burdon-Sanderson tells his fellow vivisectors however, that "all they had to secure was that perfect information might be given by Parliament."

But that is precisely the information sighed for by the anti-vivisectors, the poor suffering vivisected brutes, and by all the diseased persons throughout the World. It has been asked and waited for, for many years, and Parliament itself has been asked for that information, that *perfect information* as Dr. Sanderson puts it. But we shall never get it. All that Dr. Burdon-Sanderson, and his fellow vivisectors will succeed in doing, is to increase the groan of creation.

The following letter appeared in the *Times* of the 20th of May, 1878.

In the usual account in the *Times* of yesterday, of petitions presented, there is mention of one from the Rector of Nunburnholme against vivisection.

May I ask you to allow me to state that this does not quite correctly indicate the prayer of the petition; which was, that inasmuch as licenses for the experiments alluded to, were only granted on the representation that valuable discoveries for the life and health of men, would thereby be made, and ample time having elapsed for any result to be shewn; a return of such be forthwith required by Parliament, and until such is produced, the said licenses to be suspended;

F. O. MORRIS.

Nunburnholme Rectory, May 15th.

Dr. Burdon-Sanderson's motion was seconded by Dr. Struthers of Aberdeen. He said "*it was evident* (sic) that results of the greatest importance to *practical medicine* (sic) were flowing from physiology." An assertion frequently made at the meetings of the Association, and as frequently, (as it was on this occasion,) *totally* devoid of proof.

Sir Robert Christison then rose, and said that Dr. Rutherford "in his most admirable paper, had shewn the cause of error in previous investigations." "This" he assured the meeting, thoroughly predisposed to believe it, "was one of the fruits of vivisection."

In a few minutes, I believe the reader will come to the conclusion, that Sir Robert Christison is not a judge of the therapeutic worth of Dr. Rutherford's performances.

"The Synthesis," said Sir Isaac Newton "consists, in *assuming* the causes discovered and established as principles, and by them explaining the phenomena proceeding from them, and proving the explanation."

I have always believed, and always endeavoured to teach, that facts are the foundation of all human knowledge. But Dr. Rutherford's facts are not natural; they are not facts offered to us by Nature herself, acting in her own normal course. They are phenomena evoked from a complicated organised being, by dreadful and thoroughly artificial means. I quite admit that for anyone to suppose, that either Dr. Rutherford, or Sir Robert Christison, are not sincere, or in earnest, would be silly, as well as unrighteous. Indeed I myself believe, that if they would both of them, first master the above definition, and then test their vivisections and experiments by it, they would give up as error two-thirds of what they had hitherto taught and done, meditate in sackcloth and ashes for six months, and then recommence studying on quite a different method.

"I understand" Sir R. Christison continued, "that there was a great shyness to broach this subject of vivisection at the present meeting. I think this was a great mistake. It ought to have been made in my opinion, a prominent question. It has been made a very prominent one with the public, who are completely in error in every respect regarding it." (Applause) "Late as it is I will show you how I think I can put down the matter *at once*."

Before submitting to the reader the arguments by which the matter is to be put down at once, I would remark that as far as my knowledge goes, it is the first time in the history of "legitimate medicine," or rather as it has been called "the history of opinions," that the public has made an inroad into the soul-less body, called the Medical Association, and if it does not stop at vivisection, it will prove one of the best things that could ever have happened to suffering humanity.* But let us see how the matter is to be put down at once.

"When I went to Paris after graduating, I found all scientific Paris resounding with the recent experiments of Orfila on toxicology, by which he at once erected toxicology into the dignity of a science; and not only so, but as is the invariable result of scientific investigation, he indicated most important practical results of his expensive experiments, paid for by the French government. I venture to say that there is not in the whole history of experimental medicine, or vivisectionism, so long, so numerous, and protracted a series of medical experiments as those performed by Orfila. But we know the result. Would any one wish that these experiments had not been made? I think there can be but one answer to that question." (Applause.)

Orfila and Majendie, may justly be looked upon as the men, who above all others, *degraded science*, or rather attempted to degrade it, *to the lowest level of barbarism, cruelty and error*. Further on, a few extracts from what Sir R. Christison thinks he can call "medical experiments," will put before the reader some of the grounds on which I base my estimate of Orfila's performances.†

The next question asked by Sir Robert Christison, in the hope of putting down the matter at once, was:—

"A very short time before I went to Paris, a great noise was made in the scientific world by experiments upon animals as to nutrition by substances which did not contain azote, upon which has been founded the accurate dietetic treatment of gout and gravel."

* It has been asserted that corporate bodies have no soul.

† "If posterity remembers Majendie, he will be spoken of as a mere intrepid cutter of throats." *La Médecine à travers les siècles*. Par J. M. Guardia, M.D., Paris, 1865.

This and thousands of similar assertions brought forward by the vivisectors and their supporters, may be at once disposed of, as sailors say, by throwing them overboard. Those experiments were not vivisections at all. Nor is that all. They were not necessary, as not nearly so conclusive as what is self-evident to every physician. Gout and gravel are almost unknown among the labouring classes in the south of Europe, and not common in the corresponding class in this country. That class, everywhere, live almost exclusively in the open air, and in the south of Europe, do not eat meat more than fifty or sixty times in the year, and in some districts only once or twice, or not at all. On the other hand our statesmen and many others, city clerks for example, use their muscular system very little, and eat a good deal of meat, and among them gout and gravel is common. Rarer, nevertheless, than in the corresponding class in the south of Europe, who partake more sparingly of meat, but more abundantly of fruit and vegetables, to say nothing of very little beer, much wine, and the far greater activity of the common integument in those regions, than prevails in northern climates. That which I may be permitted to designate as regional pathology and regional physiology, is not like vivisectional experiments, misleading and cruel, but is most conclusive, accurate and *practical*. Thus again, *e.g.* softening of the brain and disease of the kidneys are comparatively rare, south of Milan. If Dr. Rutherford and his supporters, before their death relieve the brute creation of the frightful amount of suffering they *have* inflicted, and perhaps continue to inflict on them, would take the trouble to establish an international hospital, for paying patients, they would save and prolong very many lives. Every physician knows what morbid states can be benefited by a sojourn in northern and southern climates respectively.

Sir Robert Christison then gave a brief account of certain experiments made by him to test the action of oxalic acid, hydrocyanic acid, laburnum bark, and the calabar bean, ending his several observations with the usual question—"I do not suppose that anyone would say that these experiments should not have been performed"? "Applause." They were not vivisections, and have nothing to do with vivisection.

"I refer, lastly," he said, "to the investigations of Dr. Rutherford. We see what have been the results of that investigation."

The results are very easily seen, but they are of no use. I think I have shown that in several instances, the results are just contrary to the experience of whole generations of physicians, who have not "seen" but observed "the results" not in the vivisected dogs, but in the human subject. Now let the public ask this question. Sir Robert Christison approves of Dr. Rutherford's performances very much, and while endeavouring to put down the anti-vivisectionists "at once," he was applauded no less than nine or ten times. But the question to be asked is, *did he adduce one single proof that they are of any use at all or even likely to be of any use, either to man or beast?* The proofs he brings forward are mere assertions, which he hopes the public, like the members of the Medical Association, will accept, as equivalent to proofs.

"The physiological results of these experiments," he said "are extremely interesting, but their practical results are of very great importance." (Applause.)

"I do not know how the anti-vivisectionists are to answer these statements; but I shall be very glad to see the answer as soon as they are prepared to give it. (Applause.)

We do not undertake to answer "statements." In other words, we do not undertake to disprove that which has not been proved; but I, on my part also, shall be glad to give every attention to anything Sir Robert Christison may offer, as proofs in lieu of opinions and statements, whenever he is prepared to give them. But perhaps he is quite content with the "loud applause" bestowed on his "statements" by certain partisans, always ready to applaud anything done or said, provided it is called "science."

Think of the folly and cruelty of performing, I know not how many experiments on canine livers, to test the action of about *fifty* so called cholagogues. *If a cholagogue were all, or even something essentially necessary to cure diseased livers,* five or six clinical experiments on the human subjects would have been more than enough.

There is no one so dogmatic, so tiresome and worse than useless as a man who has formed a collection of facts of which he is as proud as Mr. Evergreen is of his collection of paintings, without being able to distinguish a Turner from a Titian. They are all facts, and that is all he cares to know, without the least ability to discern whether they are facts wherein Nature has been allowed to lead, or whether they are facts

in which man has done all he could to turn her aside, and violate her invariable laws, which, thank God, are never suspended to please us, or changed to suit our fancy.

"We can well conceive a practitioner at the present day who knows all the ascertained facts of physiology and pathology, and who may be notwithstanding, *inferior* to many that have lived *more than a thousand years ago*." Address in medicine by Dr. Stokes. 1865.

"This experimenter (Majendie), has nevertheless formed a school, and our biologists as they call themselves, follow him in all his blunderings. Obligated to dispense with ideas, they keep multiplying their experiments, which they mistake for experience due to their not knowing what the word observation really means. These experimenters believe that all is permitted them in their theatre, as well as in their laboratories, and while occupied with their mechanical researches, they forget the real aim of physiological teaching, even as they have forgotten the true traditions respecting that science." *Guardia op. cit.*

But the saddest part of all this has to be told. Dr. Burdon-Sanderson, Dr. Struthers, Sir R. Christison, and all the members of the Medical Association, absent or present at that meeting, justified the misuse of means and money, accepted all that was put before them regarding Dr. Rutherford's performances as practical, scientific knowledge, notwithstanding that as respects their remedial worth, *both* in the dog and in the human subject, they still remain *totally devoid of proof*. And thus, conventionally, systematically, and persistently, is the science of therapeutics confused and retarded. All the proofs they are in the habit of exacting about vivisection were amply given to them. "We *see*" said Sir Robert Christison "what have been the results of that investigation." "The physiological results of these experiments are extremely interesting but their *practical results* are of very great importance."

Without caring or waiting "to *see*" if the great body of physicians and surgeons in the United Kingdom could, at least in a measure, prove or disprove the practical results, of Dr. Rutherford's performances, the systematic errors, and traditional conventionalities that sway most corporate bodies, but especially the Medical Associations sent them all home, perfectly deluded, and perfectly satisfied.

I would now again ask, is the Inspector of physiological laboratories of any use, and are not his reports something worse than useless.?

NOTE ON ANTIDOTES.

If at any future time, it is ascertained that atropia is capable of neutralising in the human subject some or all the pathogenetic effects of physostigma or of any other substance whatever, it will not be because the atropia has acted chemically, either on the physostigma or on the organism, but because:—

When two agents, capable of generating analogous pathogenetic states in the same hystological elements of any tissue, endeavour to act each separately, the affinity they both possess for those elements and for acting in a similar way on them, forces them at once into active antagonism with *each other*, and not with the organism, which is thus freed of both the contending agents.

It is in virtue of this law which is cosmic, that morbid states may be removed. Thus, *e.g.* the pathogenetic effects of phosphorus in the healthy lungs, is an inflammatory state, similar to the majority of cases of pneumonia, caused by "cold." Hence it is, that phosphorus seldom, *if ever*, fails to cure inflammation of the lungs, because the affinity of phosphorus for the hystological elements of the healthy lungs, and its power of causing inflammation there, is so strong, that when it is administered as a remedial agent in common pneumonia, it at once endeavours to set up its own inflammation in the lungs, and, in so doing, combats, and, as it were, displaces the inflammation it finds there, and the organism thus freed, gradually, but surely resumes its own normal state. *

Provided always, that the remedial agent be not given mixed with any other ingredient, save some inert vehicle and that it be not given in doses so large as to effect the whole organism, but only those tissues and organs whose morbid condition constitutes a state for which the remedial agent has an affinity.

* "Man, while operating, can only apply or withdraw natural bodies, Nature internally performs the rest."—*Novum Organum*.

Toxic doses, accidental or otherwise, must, of course, be dealt with by other means, chemical and mechanical. The following extracts, from very many to the same purport, that occur in the medical literature of all countries, and of all ages will make the above clearer to the general reader, but not to the British Medical Association, because the truth set forth in the extracts, obvious as it is, does not happen to tally with their own sectarian notions.

In 1862 "the Gazette Médicale de Lyons announced that M. Descamps had proved that Dover's Powder was 'uniformly successful' in curing perspirations in phthisis. 'It might be doubted,' it was observed, on therapeutic grounds (!) 'whether the Dover's Powder, being itself a sudorific, would be likely to check undue perspiration,' but according to M. Descamps, 'the effect has even *surpassed* expectation, the sweating being *suppressed* from the beginning.' The perspiration was not '*suppressed*' neither was it '*treated*,' it was *cured*. Now the reason why a nail cannot occupy the place of another nail is well known, and enunciated by a proverbial axiom, and the *law* in virtue of which Dover's Powder specifically cures undue perspiration in phthisical subjects is because two similar nosogenetic processes cannot simultaneously exist in one and the same organ.

"Again," Pereira states, "that oxide of zinc administered to the healthy subject causes occasional giddiness and temporary intoxication." Dr. Marcet in his work on Chronic Alcoholic Intoxication, observes that, "it is not a little remarkable that oxide of zinc should, in some cases, produce the very symptoms it is intended to cure. Sometimes, though rarely," continues Dr. Marcet, "an apparent increase of the symptoms will occur in cases treated with oxide of zinc."

Dr. Aikin, in his chapter on neuralgia says "arsenic also appears to be a cause; at least, persons who have attempted to poison themselves with this mineral, suffer agonising pains along the course of the nerves of the limbs." *On the very same page*, alluding to the treatment of neuralgia he says, "arsenic has been recommended with confidence."

Dr. Headland states that tartar emetic, "injected into the veins, may cause pneumonia. Ipecacuanha introduced as dust into the lungs causes bronchitis and asthma, and yet tartar emetic is advantageously employed in the cure of pneumonia, and ipecacuanha is a remedy for Bronchitis."

"During the free exhibition of calomel in strumous inflammation, I have," said the late Mr. Travers, "repeatedly seen the iris take on the inflammatory action, and while the *cure* of the iris was daily accomplishing by the action of mercury in one eye, it has been common to observe the inflammation beginning in the other, as if the action produced effects diametrically opposite, upon the sound and inflamed eye." *

PROFESSOR ORFILA.

Since writing the above, I have taken down Orfila's two volumes on Toxicology, but after reading two or three pages, a feeling akin to despair, coupled with the conviction of the utter uselessness and folly of the way in which he carried on what he called "experiments," so possessed and depressed me that I gave up the intention of giving the reader a few extracts and replaced the two books on the shelf. In the course of a few hours, however, I again took the two volumes down, unwilling that my own judgment alone should influence the reader. But really his performances are very like the ways and habits of some animal giving free course to his instinct, with little or no power of discrimination. Of the zebra-ichneumon Dr. Schweinfuth says, "I found it extremely troublesome on account of the pertinacious curiosity with which it peeped into all my cases and boxes, upset my pots, broke my bottles, with no apparent object but to investigate the contents." I think Dr. Orfila's "experiments" may, to a very considerable extent, be compared to the senseless curiosity of the ichneumon.

He takes hundreds of dogs, detaches a portion of the oesophagus or gullet, makes a hole into it, forces quantities of corrosive sublimate, phosphorus; cantharides, or anything else into the stomach, notes how many hours they lived, moaned, cried, howled, etc., etc., and waits till death released the poor brutes. He then opens them and describes the lesions caused by his outrageous method of testing poisons. I intended, as I have already said, to quote four or five of his "experiments" but one or two will be sufficient, as they will enable the reader to judge of the contents of both volumes. The only portion worth anything, consists of records of cases of *accidental* poisoning in the human subject, which might have been published in a small pamphlet.

*For further information on this particular point, see Dr. Ringer's Text Book of Therapeutics.

"A small but robust little dog was hung at 6 a.m., five minutes after, four grammes of arsenious acid, in small fragments and in powder were injected into the bowel. The next day at noon a cadaveric change was observed in all that portion of the surface on which the poison had been applied. The mucous membrane had a decidedly red colour. The serous membrane, just where it turns over the bladder, presented a spot, of a darkish red colour, about the size of a one-franc piece, formed by extravasation of venous blood. All the other parts covered by the arsenious acid, had also undergone a change. But all the other tissues appeared to be in a normal condition, and it was not possible to discern the slightest change beyond the limits reached by the poison. There was thus formed a distinct line of demarcation."

He then tells us that he made the experiment on three dead human bodies with *precisely the same results*; and if anyone had asked any one living vivisector, what was the use of hanging a little dog when the experiment, whatever its use might be supposed to be, was much more conclusive, when done on the dead human subject, a ready explanation would at once be offered in the usual "scientific" jargon.

At page 163 of his first volume, he tells us that after starving a little dog for thirty hours, he forced two grammes and fifty centigrammes of cantharides in powder, into his stomach. The animal lived fifty hours, during which Orfila describes the restlessness, agitation, anxiety, cries, moans, howlings, and nausea of the poor victim of man's senseless, yet ferocious selfishness, in search of something to heal or mend the evils he brings on himself, in spite of his boasted superior intelligence.

Again, at page 80 the man who made "all scientific Paris resound" with his famous experiments, informs us that he took a dog, detached a portion of the gullet from the surrounding tissues, made a hole into it, forced through it into the stomach no less than fourteen small cylinders of phosphorus, weighing seven grammes and a half, and then describes the dreadful suffering he caused the poor animal to endure. It made desperate efforts to vomit, but could not of course succeed, as the gullet of the poor victim of man's folly and greediness for fame, had been tied below the aperture through which the poison had been forced into his stomach.

The Public can, but vivisectors and their friends are of course unable to perceive that all these and similar experiments which fill the two volumes, represent nothing that ever occurs in nature, or by accident, either in man or brute. They represent no diseased state, or "stimulate" any function in nature's own way.

The only proper way of determining the physiological and pathogenetic effects of everything, is to test them on the healthy human subject, with very small doses, repeated four or five times a day, and enjoin the subject experimented on, to continue his ordinary daily life and occupations. By these means we are able to note the gradual effect of the substance tried on the various organs and tissues. For half a century this has been insisted on by lecturers and writers on *Materia Medica* of every school and country. Dr. Ringer of University College Hospital has done a little in this way, but his experiments, and perhaps his views also are limited and cramped. The best book on *Materia Medica* in the World, is ALLEN'S *ENCYCLOPEDIA OF PURE MATERIA MEDICA*, Borwick & Co. New York.

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